LEPREAU 2

ENVIRONMENTAL IMPACT STATEMENT

A REVIEW

by

DR. CHRISTOPHER GARRETT

Dept. of Oceanography
Dalhousie University
Halifax, Nova Scotia
B3H 4J1 Canada

October 1984

1. OUTLINE

This short review of the Lepreau 2 EIS will concentrate primarily on the thermal plume modelling and related studies, as described in the EIS of May 1984, the Supplemental Information of September 1984 and the Supporting Document 1(9), Memorandum on Thermal Plume, from Dr. Paul Wisner, dated 28 August 1984.

The review will first discuss the variety of factors that must be considered in a discussion of the thermal plume, and then review the extent to which the treatment of the problem in the EIS is adequate. This will lead on to recommendations of the work that will be required if improved predictions are required. Various other issues related to the thermal plume will be discussed briefly.

In Section 7 I shall deal explicitly with questions 1 to 6 posed in the letter of 4 October 1984 from Dr. M.H. Sadar, although my answers will be implicit in the earlier sections.

In Section 8 an attempt will be made to address the questions, raised in the letter from Dr. Sadar cited above, of the dispersion and deposition of released radionuclides.

2. PREDICTION OF THERMAL PLUMES

The discharge of a jet of hot water into the environment typically leads to a very complicated fluid dynamical problem in which most of the following factors are likely to be relevant:

a) the characteristics of the discharge (temperature, speed, outfall geometry),

b) the density difference between the discharge and the ambient

water,

c) the stratification of the receiving water,

d) the motion (mean current) of the receiving water,

e) the turbulence in the receiving water (as opposed to the turbulence generated by the jet itself when discharged into water that is either stagnant or in laminar flow),

f) time-dependence of the discharge or, more likely, in the characteristics of the receiving water,

g) the geometry (water depth, bottom slope, discharge depth, etc) of the problem, and boundary effects such as surface cooling,

h) fluctuations, particularly as related to the meandering of a plume. The predicted excess temperature field relative to the centre line of a plume will differ from that, for fixed positions, averaged over an ensemble of realizations of the plume. (In other words, one should ask a modeller whether a prediction of an excess 1° C at a particular point means that the excess temperature there is always 1° C under the same conditions, or whether it is, for example, 0° C on half the occasions and 2° C for the other half).

In the present problem all of these factors are relevant, and there are three main dangers:

(i) that one uses a model which apparently allows for everything, but in which it is unclear which are the key factors and hence it is difficult to determine the sensitivity of the results to poorly-determined parameters,

(ii) a simplified model which neglects key processes is used, and

(iii) that predictions are based on a comparison with other situations which are not, in fact, comparable.

3. MODELLING AND PREDICTION IN THE LEPREAU 2 EIS

The discussion of the thermal plume in the Lepreau 2 EIS and supporting documents is remarkably poor. The models used for prediction or scaling are, by and large, inappropriate, parameter values used in the models are generally not given, key proceses are not identified and predictions are made only for specific environmental conditions or based on observations for two days, rather than being for general, or "worst case", conditions. More specifically:

(i) The "hydrothermal simulations" are based on a two-dimensional numerical model which appears to assume vertically-uniform, though horizontally-varying, temperature. This hardly seems appropriate, and certainly has not been justified. No information is given on the flow field or diffusion coefficients used in the model. I suspect that the diffusion coefficients used were constant, and that the predictions were thus for the ensemble-averaged temperature field rather than an instantaneous plume (see comment 2 (h) above). This has not been discussed.

(ii) The predictions for the Lepreau 2 thermal plume are largely based on scaling up the very limited field data according to the formula presented (with r = 3/2 instead of r= -3/2) on p. 6-10 of the EIS.

Dr. Wisner tries to justify this formula in section 2.2 of his memorandum and by reference to the documents he includes. The justification is either unclear, inadequate or based on incorrect premises:

a) the work cited on pp. 38 to 39 of the report by Edinger et al. does not explicitly show that A_n is proportional to $Q_p^{3/2}$. The formula 5.37 in fact suggests that, via the Froude number dependence, $A_n \propto Q_p^{-1.3}$ for a jet entering stagnant water.

b) the accompanying formula (5.36) gives r = -2.936, rather than the -1.5 used.

c) none of the situations cited on p. 38 include stratification of the ambient water.

d) the result $A_n \propto Q_p^{3/2}$ in the formula (8.3.11) to which Dr. Wisner refers is for a heat source in a constant current. It may be irrelevant at the conditions of high and low tide which receive some of the emphasis in the EIS, and at which the dispersal of the thermal effluent might be least. (Indeed (8.3.11) would have $A_n \rightarrow \infty$ as $u \rightarrow 0$ at slack tide!) Moreover, the mixing coefficients have been taken as constant, which is certainly inappropriate for a prediction of the instantaneous plume.

e) the result $A_n \propto Q_p^{3/2}$ is also justified by reference to the data plotted in Figure 8.3.4 of the report by Edinger et al. The relevance of this is again limited, at best, to situations in which there is a mean flow.

f) if the presence of a current is vital to the validity of the scaling formula used, then this formula should also show the dependency of A_n on this current, and predictions should be made for different current regimes as well as for those that occurred on the two occasions on which field observations were made.

g) the justification of r=-3/2 from Figure 8.3.3 is again dependent on the presence of a current, and uses a scaling that

would have $A_n \to \infty$ as $u \to 0$.

h) it is worth pointing out that the transformed areas presented on p. 6-11 of the EIS give a value of r closer to -4.

4. THE THERMAL PLUME IN A STRATIFIED OCEAN

As discussed in Section 3, the modelling and scaling arguments are unsatisfactory even without the added complication of density stratification in the receiving water. A primary issue now is to determine the level to which the plume will rise or sink.

It certainly seems plausible that the sinking plumes experienced in lakes are associated with the 4°C density maximum of fresh water. For salinities above about 25°/oo the density of seawater decreases monotonically with increasing temperature. However, the nonlinearity of the density as a function of temperature can lead to some surprises. For example, the mixture of two water masses of equal density but different temperature and salinity is denser than either parent and will sink. The phenomenon is known as cabbeling. Indeed it is even possible to mix two water masses of different densities and obtain a mixture slightly denser than both.

This possibility is recognized by Dr. Wisner in his analysis of two special cases (see Figure 1 of his memorandum). The problem can be studied more effectively and generally through the use of the standard oceanographic T-S diagram. Given the relative depths of intake and outfall, a sinking plume seems unlikely, but the analyses presented so far are inadequate and

need to be extended.

5. RECOMMENDED STUDIES

I have suggested above that the studies, to date, of the thermal plume that will originate from two CANDU reactors at Point Lepreau have been muddled, inappropriate and unreliable. This may not, in fact, matter very much. The consensus of international studies seems to be that in temperate latitudes thermal discharges into large bodies of water do not generally create any serious problems (see "Thermal Discharges in the Marine Environment", Reports and Studies No. 24 of the IMO/FAO/UNESCO/WMO/WHO/IAEA/UN/UNEP Joint Group of Experts on the Scientific Aspects of Marine Pollution - GESAMP, published in 1984 by FAO in Rome). Moreover, it is difficult to think of a more suitable receiving water mass than the cold and turbulent Bay of Fundy!

However, it is clear that the problem is complicated by the presence of significant stratification in springtime, and that this, at least, calls for some further study. Some questions here are

(i) How much mixing with ambient water occurs before the jet has time to respond significantly to the buoyancy forces on it? (This is also a good general question to be asked for unstratified receiving water).

(ii) Using the T-S diagram and the annual T-S cycle described by Bailey et al., J. Fish. Res. Bd., Canada, 11, 1954, (reproduced as Figure 4.6.2 of the Lepreau 1 EIS) or updated or more local

data, where will the mixed water tend to settle out?

(iii) Is there any possibility that in a stratified, and hence rather less turbulent, regime, any warm water from the outfall could enter the intake? (This does indeed seem unlikely, but the possibility needs further discussion).

6. OTHER ISSUES

One of the purposes of an EIS is presumably to provide information that helps the general public appreciate, as simply as possible, the environmental consequences of a project. In this regard it seems to me to be very important to put the issue of "thermal pollution" into perspective. This can be done by comparing the waste heat discharged to the ocean with the heat typically transferred to the ocean by solar radiation and air-sea exchange processes.

The waste heat from two reactors at Point Lepreau will be about 3,200 MW. This may be compared with an average heating of the Bay of Fundy (by sun and air) of about 200 Wm^{-2} in mid-summer, which would amount to 3,200 MW for an area 4 km x 4 km. The waste heat is thus seen to be insignificant on anything but a local scale.

The ultimate fate of the waste heat should be discussed further. It seems plausible that much of it will be transferred to the atmosphere as latent heat of evaporation, along with associated water vapour. This may lead to increased fogginess and ice deposition in the Point Lepreau area. The issue may be a very minor one but is not addressed in the Lepreau 2 EIS.

7. REPLIES TO SPECIFIC QUESTIONS IN DR. SADAR'S LETTER OF 10 OCTOBER 1984

1. Present your opinion, regarding the appropriateness of the plume model for the Lepreau situation.

The models seem to be largely inappropriate, though this may not matter.

2. The EIS and subsequent submissions by Maritime Nuclear present predictions of plume performance based on empirical data gathered from two sampling events. Is this data adequate support for the kinds of predictions made? What level of certainty can be associated with predictions based on this limited monitoring effort?

The data are not adequate for validation of the models. Even if they were, the models have not been used properly to investigate the whole variety of possible situations that can occur. Nonetheless, the limited observations do suggest that mixing near the outfall is rapid and give a rough idea of the size of the region of elevated temperature that might be expected.

3. Does the analysis carried out in the EIS and supporting documentation hold true for all probable conditions expected in the vicinity of the Lepreau development? Specifically, are the conclusions reached valid under winter conditions?

Insufficient attention has been paid to the variety of conditions that might be expected. The predictions for winter conditions are not based on plausible arguments.

4. Are there any environmental conditions which would cause the plume from a two unit operation to impinge upon the bottom. If so, what would be the frequency of such events and what would be the impact on benthos?

It seems unlikely that the ambient conditions at the cooling water intake and outfall will permit the plume to sink, though further study is required if the question is thought to be

important. Such a study would follow the outline in Section 5 of this review. Note also that the plume need not sink in order to impinge on the sloping bottom.

5. In consideration of the information presented thus far by Maritime Nuclear, and your responses to the above questions, comment on the need for, and utility of a monitoring program for defining the plume under actual operating conditions. Conceptualize such a monitoring program.

I doubt that the thermal plume presents a significant threat to the biota (though I await the judgement of more qualified scientists). In that case a monitoring program does not seem to be justified if one considers only the Point Lepreau situation.

However, a case could be made that the present situation should be monitored in an effort to improve models for application in situations where the thermal plume might present more of a problem.

The design of a monitoring program would require that one first think through the kinds of questions introduced in Section 5 of this review, but I suspect that a judiciously placed array of moored thermistor chains and at least one current meter could be very valuable. It would certainly seem important to use moored instruments rather than relying solely on a few surveys from ships.

6. Based on your analysis, does the rationale for evaluating significance of impacts of thermal effluent (p.6-18 fol. of EIS) represent a valid approach?

I will leave this question for Dr. Boyd to answer. My superficial impression is that the estimates of entrainment losses are adequate and that the effect of the thermal effluent

will be small, even though estimates in the EIS are based on predictions for the thermal plume which are unreliable.

8. THE FATE OF RELEASED RADIONUCLIDES

I lack the expertise to comment authoritatively on the problems of released radionuclides and the radiation dose to man that may result. Nonetheless, I shall make a few comments as a "semi-layman".

(i) I am slightly alarmed that a misprint by a factor of 1,000 can occur in the EIS on p. 5-17. Presumably the figures should be 10^6 for 1mSv or 10^9 for 1μSv.

(ii) On p. 4 of "Supplemental Information" we are told that detectable amounts of Cs-137 and Sr-90 may be emitted, but not what fraction of the DEL's these discharges may be.

(iii) We are also told on p. 4 of "Supplemental Information" that "The study of mobility and accumulation of radionuclides in the marine environment near Point Lepreau is a research question requiring long term studies". This is only partly true; for both routinely emitted radionuclides and those that might be released in an accident I believe that reasonable estimates of their fate could be made from experience elsewhere and some knowledge of factors such as local sediment composition.

In particular, I would have thought that the fate of the comparatively very large releases from the Windscale plant into the Irish Sea would provide valuable guidance.

In specific response to the questions of Dr. Sadar's letter of 4 October 1984:

(1) Evaluate Maritime Nuclear's responses regarding the fate of

radionuclides released to the marine environment and comment on their validity.

The response is not entirely adequate for the reasons given above. Specifically, expected releases of Cs-137, Sr-90 and other radionuclides should be related to DEL's, and more discussion of accidental releases might be warranted, possibly using experience elsewhere.

(2) Comment on the postulation that sediments passing the Point Lepreau Generating Station will absorb radioactive particles and be subsequently accumulated in the Passamaquoddy region. Should you conclude that there is "reasonable" likelihood of active deposition of reactor-released radionuclides, predict in general terms the possible ecological implications of such a phenomenon.

From my limited knowledge of this topic (based on some interaction with geochemists) it does seem possible that sediments passing Point Lepreau will absorb some radioactive particles. The degree to which this occurs will depend on various rate constants. It is also possible that the sediment will later be deposited in Passamaquoddy Bay.

However, it is important to realize that this scavenging process is thought to be reversible (see, for example, discussion and modelling in GESAMP's Reports and Studies No. 19, "An Oceanographic Model for the Dispersion of Wastes Disposed of in the Deep Sea", published in Vienna in 1983 by the International Atomic Energy Agency).

Thus, even if rate constants are large enough for some radionuclides to be scavenged from the water in a region of high concentration near the Point Lepreau outfall, they will tend to be released again as the sediment encounters cleaner water.

Even in the event of a large accidental release of highly

reactive (easily scavenged) radionuclides and subsequent deposition in Passamaquoddy Bay, the radionuclides would later be released and would probably leave the Bay on a time scale much less than the radionuclide's half-life.

These statements should, of course, be checked by a competent geochemist.

(3) Is it possible to monitor the dispersion trajectories of radionuclides released from Point Lepreau into the marine environment? If so, present a conceptual monitoring program and comment on its utility.

Emissions from the plant should, of course, be monitored. I doubt whether it is worthwhile (or even feasible) to monitor the dispersion of small routine releases, but perhaps a plan should be ready for the monitoring of any large accidental release should one occur. I am not competent to design such a plan.